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## GENETICS AND ANTHROPOLOGY[1]

By Professor C. H. DANFORTH

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WHILE all scientific activity might well be regarded as a single enterprise, human limitations are such that individually we can reach the boundaries of knowledge in but few places. This handicap is offset in some measure by the fact that acquisitions laboriously achieved in one domain may be taken over in whole or in part by workers in other fields. Anthropology has profited much in this way, having drawn heavily on the physical sciences and mathematics as well as on biological specialties and the humanities. Through borrowing and mutual interchanges the forefront of scientific progress is kept from becoming ragged.

A survey of journals issued during the last few years shows that one of the important recent anthropological contacts has been with the field of genetics, which to anthropologists as a group is no longer quite *terra incognita*. Especially is this true with some of our European colleagues, among whom human genetics is coming to be regarded as a natural part of anthropology. In our own journals during the past ten or fifteen years there has occasionally appeared an article with a distinctly genetic background, and in several important monographs genetic aspects have received able consideration. Conversely, an easy-going assumption of a direct and immediate effect of climate or custom on the human germ-plasm has become less frequent. So it could hardly be said to-day, as with some justice it might have been only a few years ago, that the platitudes of genetics are the heresies of anthropology. But in place of the violent revolution in anthropological thought which some great exponent of the genetic point of view might have stimulated, we are witnessing a slow and

[1] Address of the vice-president and chairman of Section H—Anthropology, American Association for the Advancement of Science, Boston, December 29, 1933.

subtle infusion of genetic ideas suggesting one of those peaceful human infiltrations which have supplied anthropologists with many of their problems.

It is probably correct to state that the most far-reaching contribution of genetics has been the theory of the gene. One might say that the gene is to some of the biological sciences what the atom is to the physical sciences. Genes are believed to reproduce themselves exactly with each cell division, and so may be regarded as in effect permanent units which pass on from one generation to another unchanged by the vicissitudes of reproduction, combining and recombining, often with effects suppressed, but with no loss of individuality; much as atoms enter into one compound and then another but may always be recovered quite unchanged. Genes, like atoms, undergo mutations; and in the investigation of these respective units the logic, and in a measure the techniques, of the physicist and the geneticist have been strikingly similar. For present purposes we may think of the genes as minute, self-perpetuating particles located in the chromosomes and transmitted by them from one generation to the next. It is they that contribute the hereditary factors in the development of individual constitutions. What is of prime importance here is that these determiners of traits are particulate rather than diffuse or ethereal elements, that they are self-perpetuating and relatively immutable. This concept finds ready application in physical anthropology.

It is sometimes objected, however, that while the genetic approach may be a profitable one in studying some human traits, it must fail with others because of the multiplicity of genes and the complexity of their interactions. This objection is a very real one. As the number of genes influencing a trait becomes large, there is an approach to that "infinite number of agents" often assumed by the mathematician, and so it does not seem probable that genetics will completely emancipate certain phases of anthropological work from the relative crudities of the statistical method. But it is likely to contribute much toward clarifying postulates on which mathematical analyses are based. A hopeful trend is indicated by Laughlin's paper on the "General Law of Heredity" and such recent work as that of Fisher, Wright and Haldane. But whether genetics contributes much or little in this respect, the fundamental idea of a particulate rather than a diffuse physical basis of heredity is likely to stand.

Some of the more interesting implications of the genetic point of view may be indicated by considering in cursory fashion a few subjects of anthropological concern without being diverted for the moment by qualifications and reservations which might suggest themselves. A final estimate of the value of these considerations must be reserved for the future.

### Race Crossing

It is still customary to speak, and perhaps to think, in terms of common fractions when referring to racial inheritance. Some of us are not quite free from the notion that the "blood" of two races is blended in the offspring like two fluids in a beaker. If a mulatto were symbolized by a half-and-half mixture of milk and black coffee, the descendants of mulattoes, it is commonly assumed, should be represented by combinations of these fluids in amounts proportional to the number of white and colored ancestors. Terminology consonant with such a conception is convenient, so long as we are not subconsciously deluded into thinking that the simple fractions have a definite meaning with reference to particular individuals. Genetics has gone far to undermine any notion that a race cross is like the mixing of two liquids. Germplasms, it maintains, do not blend; their particulate elements mingle and, uncontaminated, segregate again. In any individual which is produced there may indeed be many kinds of blends and compromises in the effects which the genes condition, but the genes themselves remain unchanged. Nothing in genetics is more generally accepted than this.

So it would appear quite possible that a mulatto couple might produce a white child without a single Negro gene and a Negro child with no white genes. In view of the 48 chromosomes of man and with no crossing-over of genes between homologous chromosomes, the chances for the occurrence of two such unrelated sibs would seem to be approximately one in $7 \times 10^{28}$. If this is considered as negligible, it should be recalled that the calculation takes into account every chromosome and all the genes. It is probable that the vast majority of genes are the same in both whites and Negroes, which greatly reduces the odds against the mulatto couple producing a pure Nordic child. But, even so, the chances of such an event must be measured against figures with a magnitude to which most of us are unaccustomed. However, if we neglect racial complexes as a whole and consider merely individual traits, the situation is quite different. Many of the latter are conditioned by only a few genes, or even a single one, and naturally such traits do frequently segregate.

We should consequently expect our mulatto couple to produce children with prevailingly hybrid traits, but among them some who might be purely Negro or purely white in certain particular respects. Examples of this are often met. To cite a single one: I know a man who is as black as human beings often come, but in his ancestry there is much Indian and white

blood. The shape of his head is unlike that of most Negroes and there are other features which are not characteristically associated with his color. The reasonable inference is that when his existence was initiated he received most or all of the Negro genes for skin color, while those for different traits came largely from the other two great races upon which he could draw. To describe his inheritance in terms of common fractions would be to make only a crudely statistical approach to the truth. Indeed, his sibs, with the same fractional formula, are of quite different types.

The expected, and on the whole the observed, result from mingling of peoples is a breaking up of racial complexes, but not of genetic traits. Long ago, before modern genetics could be considered in connection with anthropology, de la Pouge emphasized this point, stating in effect that all the types in Europe have existed for ten thousand years and intermarried continuously, but to-day are not one bit nearer than in the beginning to a common fixed type of mixed race. Such considerations impress one anew with the importance of the trait as contrasted with the complex in dealing with race and race crosses. In other words, our study promises to become more fruitful as it becomes more individual.

At the same time the genetic outlook affords a warning against the indolent use of some common methods. For instance, when the progeny of a race cross, or any other variable group, shows a rather wide dispersion in respect to a trait, the prevalent tendency is to arbitrarily seriate the observations into artificial categories such as "1, 2, 3," etc. The danger that inheres in this procedure may be indicated by reference to a field in which knowledge is relatively precise. In some of the laboratory animals where the heredity of hair color is well understood, there is to be found what might appear to be a continuous series of shades from pale yellow to pure black. Such a series would admit of several different and seemingly reasonable artificial subdivisions, but there is only one classification that coincides with the genetic background. If the hair colors of these animals had from the first been classified as arbitrarily as those of man commonly are, the records would no doubt be even more copious, and the significant facts still well concealed.

For the study of race crossing, genetics brings to anthropology a revised, and in many respects an illuminating, outlook. It also brings a demand for a burdensome and difficult addition to earlier techniques. Furthermore, it raises several general and rather fundamental questions relating to the underlying nature of the races which cross. Does a race represent anything more than an aggregate of independent traits? Have we reached the essentials when we identify the genes by which it differs from another race? There is much evidence that the latter may be so, and I know of little specifically to the contrary, but conclusions must be drawn with caution. The possibility of some underlying "matrix," which, while responding to the genes, is itself responsible for racial and specific characteristics, has not been wholly eliminated, however improbable its independent existence may now seem.

### RACIAL DIFFERENTIATION

The problem of race crossing and that of race in a more special sense is essentially the same. It is a commonplace that "there are no pure races." In other words, "purity of race" is only a relative term. This is an important fact, often emphasized. Still we do find that whole sections of the world are, or were, given over to one racial type and other sections to different types. Such local individualization of races presents one of the great problems of anthropology. One can only wish for more dependable information as to whether the main races of mankind and their major subdivisions were in fact biologically best adapted for their original homelands and, if perchance they were, then by virtue of which of their distinctive traits. One would wish to know how many racial traits are purely incidental to, and in a sense by-products of, traits of more crucial import—preserved and developed because some of the genes that help to determine them are also necessary to the production of genuinely vital characters. These are questions to which answers will probably come slowly, but of the modes of attack at present available, the genetic one seems most promising, since a particulate type of heredity appears to be the most favorable for the concentration and preservation of desirable attributes.

From an analysis of influences and tendencies active to-day and undoubtedly operative from time immemorial, we may hope to gain some inkling as to the manner of racial development. In the study of these problems and the principles which they involve, one need not go to remote places. The traits of civilized man are no less real than those of savages; indeed the former, reproducing at a rate of scarcely more than three generations to the century, may be even a bit closer to the common ancestor than are some "primitive" groups which reproduce at a more rapid rate. The significant contribution of genetics at this point is the notion of permanency of the gene. We may in fancy take a census of the genes in one generation and know that in the next we shall find the same ones, or their exact equivalents, all present but regrouped in new combinations. In other words, genetically each generation is in the aggregate, like

the preceding; only individuals change as the *abc* and *def* of the parents become the *abf* and *dec* of the offspring, through endless groupings and regroupings of unchanging factors. Such at least would be the case (except for rare mutations) if all individuals were to reproduce at the same rate. But if reproduction becomes differential with reference to any trait, the ratio between the genes for that trait and their allelomorphs is altered. This seems to be the key to racial change. Genetics supplies what the older theories of natural selection did not offer, a basis for calculating the nature and rate of change. Useful methods for such computations have been devised and bring into relief some aspects of demography that might not otherwise have become apparent. Particularly they point to a conception of human populations in terms of their total germ-plasms, in which each gene and its allelomorph tend toward a state of random distribution. But such a distribution does not make for true uniformity; it merely decreases the frequency of extreme types. Uniformity, we are led to expect, will come only through differential reproduction with the elimination of gene lines.

Students of genealogy and of biological descent have heretofore used much the same criteria of relationship, but we must now differentiate between descent that is merely genealogical and that which is also genetic. The reason for the distinction may be indicated by considering the source of an individual's chromosomes. From each parent he receives 24. Of these each grandparent on the average contributes 12, each great-grandparent half that number, and so on until in the sixth generation one has more grandparents in his genealogy than he has chromosomes in his cells. Obviously, if there has been no "crossing-over" of chromosomal elements, some ancestors in the sixth generation back have been completely eliminated in a genetic sense. They are only empty forms that mean nothing in his heredity. Indeed the chances are that at least one ancestral line will be eliminated even before six generations have passed. Not all my father's ancestors are mine. It follows from this that one may have no more than 48 genetic ancestors in any one generation. If it should develop that there is much crossing-over of genes between the chromosomes in man, the fact would serve to make these figures somewhat less striking but scarcely less significant. The essential point is that genetically a man can be descended from only a very limited number of his genealogical ancestors.

Conversely, a man is entitled to claim real ancestry for only about three fourths of his descendants in the sixth generation, with possibly a small bonus, depending on the frequency of genetic cross-overs. Ultimately, if there is no differential reproduction, he might become the genetic ancestor of descendants to the number of 48 times whatever figure represents the ratio of the population in his own generation to that of the generation in question. But if his descendants do not scatter widely, they are likely in time to intermarry, despite civilized prohibitions or the most complicated taboos of the cleverest savage, and sooner or later genealogically remote grandchildren will be produced who are genetically more closely related to him than are most of his nearer descendants. Such partial "reincarnation" is often called "reversion." When it is frequent, so that many individuals not obviously related show the same traits, it implies paucity of genetic ancestry and suggests that only a few patriarchs supplied the genes for traits which now appear in oft-recurring combinations.

These genetical considerations, reinforced by various data of other kinds, suggest two modes of racial origin with results which need not in the end be essentially different. One produces individuals who, because of the fewness of their genetic and original genealogical ancestors, have relatively few gene lines which differ. Such, no doubt, are some of the distinctive groups within the three or four major races. The other, starting with many and diverse genealogical ancestors, produces a homogeneous group through differential reproduction and its resultant segregation and realignment of genes, to the end that ultimately these too have relatively few (but originally diverse) genetic ancestors. Such perhaps are some of the Pacific Islanders and, incipiently, a few modern nations. The thesis which genetics may be thought to present is that, as racial groups are found to be homogeneous, their genetic ancestors may be inferred to be few.

### Inter-racial Distribution of Traits

Delimiting of races is notoriously difficult. In general the fewer the diagnostic features, the more widely distributed, or the more "artificial' the group which they define; while the more numerous the criteria, the more uncertain and variable the racial limits. Particular types of build and hereditary peculiarities of various sorts appear again and again in different races. From almost any cosmopolitan group one can select black, white and yellow subjects with features in common. By employing criteria other than the conventional ones, it would be possible to classify some white men and some Negroes in a group from which other white men and other Negroes would be excluded. Indeed, it does sometimes happen that racial boundaries proposed by one student cut across those favored by another. It has even been suggested, half seriously, that no criterion based on more than a single character can be an infallible index of race.

In short, while there may be constellations of characteristics that tend to be associated, there are nevertheless many traits which are not limited by ordinary racial boundaries.

Genetics suggests that there are three ways in which the same sporadic trait might come to be present in different races: (1) It might have belonged to some common ancestor of these races, (2) it might have been introduced by race crossing, or (3) it might have arisen independently in the different groups. Probably each of these explanations is the correct one in certain cases. The genetical theory of population leads to the expectation that in the absence of differential reproduction, gene lines will continue with about the same incidence through innumerable generations. In the differentiation of races many gene lines may have escaped the effects of natural selection. It is not surprising that there are a number of traits appearing with varying frequency both in ourselves and in the anthropoids which are best explained on the assumption of a community of gene lines. In the races of man with their immeasurably long period of common descent, there might be expected to be a large accumulation of heritable variations which have never been of sufficiently vital importance to insure either their fixation or their elimination. Probably no one is more forcibly impressed by the great array of traits, which show scant respect for racial boundaries, than is the anatomist who has occasion to dissect bodies of diverse types. Many of the variations are known to be hereditary, so it might be said that in the dissecting room the brotherhood of man is insistently proclaimed.

The possession of a few traits in common need not necessarily imply recent or even relatively remote race crossing. Nevertheless, from the time when races of many began to be emancipated from their respective faunal limits, crossing has undoubtedly been an important factor in the dissemination of such traits as may once have been limited to particular groups. A geneticist would expect each "infusion" of foreign blood to bring into a community some genes which might differ from those of the original stock. In succeeding generations these new genes would become dissociated from each other and ultimately be disseminated at random through the germ-plasm of the community. Dahlberg has developed, in a way that is full of suggestiveness for anthropologists, methods for measuring the rate of such dissociation in human populations. With repeated, even though infrequent, infusions from different sources, followed in each case by an ultimate dissociation of genic complexes, the germ-plasm of the original group would become more complex and more cosmopolitan. Thus there would be provided a foundation for the sporadic appearance of some traits, even though they might be complex ones, in several different racial aggregates.

The genes 0, A and B, which are instrumental in the determination of blood groups, exemplify this type of dissemination. While the blood groups as traits are by no means unique in the nature of their distribution, they have fortunately caught the fancy of investigators in many lands, so there are now three genes on whose present distribution and probable history we are beginning to have a significant amount of information. Unfortunately there is a rather widespread misconception as to what anthropologists may expect from these genes. It has been hoped by many that a constant association would be found between one or another of them and other racial traits—that they would prove diagnostic of race. But this is the last thing to expect. One would suppose rather that these genes, like all similar ones, long since became completely dissociated from any original genetic complex that may have been introduced into modern strains in their remote past, and this supposition agrees with the findings. What such genes do offer is the possibility of tracing courses of dissemination in gene lines, as is indicated by some of Bernstein's analyses. When other genes shall have been studied with the same intensity, we may look with confidence for a picture of racial origins based on internal evidence that will probably be more accurate than any that can now be drawn from external evidences.

In the possibility of independent origins, or parallel mutations, the physical anthropologist and his colleagues in the cultural field find grounds for mutual sympathy. The same mutation like the same idea may perhaps arise more than once, and in far distant places. That some mutations do indeed recur has been established beyond question. Further it appears that different ones show widely varying frequencies. Some which produce teratological effects are relatively common, others must be exceedingly rare. For example, to recur to the blood groups, the infrequency or possible absence of B in American Indians suggests an extreme rarity of mutations from 0 or A to B. The question of parallel mutation in man needs to be explored more fully. It may not prove of very great importance in the study of racial development but, like parallel evolution, it must not be forgotten.

One of the most difficult obstacles to be met in any attempt to interpret racial origins and the distribution of traits in terms of genes is found in the wide-spread assumption that races have an intangible and elusive quality which can not be resolved into discrete elements. Whether this supposed quality is partially imaginary or not, there is one further aspect of genetics which should be considered in connection with it. It has been found that the expression of certain genes

is contingent on the presence or absence of others which may serve to enhance or suppress the usual effects. Conversely, a few genes may influence the manifestation of many. Traits associated with sex furnish a case in point. The sexes differ somewhat qualitatively in a few respects and quantitatively in almost every respect, even to the incidence of morphological variations. Sex is generally conceded to be due ultimately to a difference of only a few genes. Racial differences show a certain parallel to sex differences—pronounced divergence in a few traits, slight variation in degree or frequency of many. It would appear from this that the distinctive genes of a race, like those of a sex, tend to modify in some measure the effects of those more widely distributed genes with which they come to be associated. To this extent there is a possible genetic explanation for some elusive racial "essence." This supposed quality led a few years ago to suggestions of a possible endocrine basis for racial differences. But the existence of any such basis has not been well established, and a primarily genetic explanation still seems the more plausible. This is particularly so since some comparative evidence, and a little from human sources, indicates that in so far as factors like endocrine secretions are involved at all in the production of racial peculiarities, the real differential is not so much in the hormones as in the responses to them. This is an inviting field for further study. Thus far the evidence indicates that genic factors are the important ones in establishing those generalized features which may seem to characterize a particular race.

## Morphology

To-day the simple confidence which we once had in certain morphological concepts is replaced by a conservative scepticism to which a growing appreciation of genetics has contributed in no small degree. We can no longer regard homology as the simple thing we had thought. Instead, some think of it as distinctly relative and best measured by the number of genes that two or more individuals have in common. This outlook leads to the recognition of a class of conditions in which apparent homologies may be essentially spurious. Hyperdactyly will serve as illustrative of such cases. Man and other higher vertebrates normally have at most five digits on their limbs. Nevertheless, families with an excess number occur in many mammalian and avian species. Embryologically, the condition is apparently due to an early tissue overgrowth with a resultant segmentation into an increased number of digital rudiments. It is apparently relatively simple genetically, as one might expect, to effect changes that will be of a merely plus or minus nature, and mutations which do that are rather common. Such a change affecting the hand or foot in its initial stages of development would seem to offer an adequate explanation for hyperdactyly and leave no ground for homologizing the extra toes of men, cats and chickens. Nevertheless, two leading students of the foot have recently maintained that the presence of an extra toe is to be regarded as reversionary and indicative of a phylogenetic loss in normal lines. Such a conclusion fails, it would seem, to take into account genetic and other experimental findings.

Of all morphological concepts, perhaps the "biogenetic law" has suffered most from the rise of genetics. The notion which it involves has been a stimulating one with, perhaps, elements of validity, but it has proved a treacherous guide. So long as recapitulation was considered a "law" one could argue with Brandt that if Pithecanthropus had a beard, and was the ancestor of modern man, present-day infants would necessarily be bearded at birth. But long ago it became apparent to geneticists that the child develops as the parent does, not because it is his child, but because both parent and child come from similar germinal anlagen. The "biogenetic law" in its orthodox form is based on a failure to accept this point, a kind of hangover from the time when germ cells were supposed to receive their characters from the parental soma. From the present-day point of view, the situation appears rather more simple. So long as a stock is relatively unchanging, the offspring will develop as did their parents—will "repeat" the ancestral ontogeny. Depending on whether mutational changes affect processes occurring early or late in development, the "repetition" will be more or less modified. This, together with a possible tendency for genes to get in their effect as early as possible (Haldane *et al.*), may be all there is to the "biogenetic law." When its indications are positive, it may be useful; when they are negative, it is likely to be without significance. A single illustration may serve to emphasize the deficiency of the theory of recapitulation in human studies. All other Primates so far examined have a few hairs on the terminal segments of the fingers near the base of the nail. There can be little doubt that the ancestors of man had hair in this region long after they became primate and even anthropoid, so according to the "biogenetic law" at least rudiments of these hairs should occur in the human embryo. None have been found. There are many comparable cases, and also those in which a suppressing or modifying gene becomes effective after the first steps in development have been taken.

In connection with any criticism relating to ideas of homology and recapitulation, however, it could not be emphasized too strongly that the genetic point of view brings no threat to morphology. It does pro-

vide a possible escape from some of the impasses into which the older types of morphological reasoning led us.

Finally, if we ask just what genetics can contribute to an understanding of human phylogeny, the answer for the present must be tentative. Perhaps its greatest contribution is a point of view. Some botanical geneticists claim much for it in their field and write, for example, of such matters as the hybridization and genetics of Pleistocene roses. The cytological picture in man does not encourage the hope that anthropologists can soon parallel the botanists in this field. Frequently "Neanderthaloid" specimens are exhibited or described, and occasionally one meets in the flesh an individual who may show some approach to a preconceived notion of what Neanderthal man was probably like. Rather detailed measurements of a striking example of this sort who appeared a few years ago showed, in spite of his archaic aspect, a decided balance on the side of modern man. Do such cases really have any phylogenetic significance? Before answering this question we must have more information than at present. One naturally asks first if there is evidence of heredity in these "Neanderthaloids." In the case just cited there may have been, for the subject claimed close resemblance to a deceased brother. Granting that such a set of traits does in fact represent an hereditary complex, and one suggestive of Neanderthal man, it would still be hardly justifiable to assume that it implies descent from *Homo neanderthalensis* until the component elements of the complex have been determined and their distribution ascertained. But there is hope, in view of the increasing amount of available Neanderthal material, that enough may be learned about the types and range of variation in this form, as well as in *H. sapiens,* to warrant sound conclusions on the now debatable genetic relationships. Not until more has been done in this direction can opinions be passed with much assurance on a whole range of manifestations which are now rather casually assumed to be "reversions"—or, as the geneticist might prefer to express it, products of a recombination of genes long since dissociated.

### CONCLUSION

It would be unwarranted to claim too much for the contribution of genetics to anthropology. Genetics can not solve all the problems; it may give a final answer to none of them, but it does provide a point of view and a methodology which are of fundamental significance, and it furnishes an orientation which brings into relief a fresh and stimulating array of new problems. This revitalizing influence, even more than the immediate direct accretion of fact and method, may in the end prove to have been its greatest contribution. To widen the horizon and provide new problems, or a fresh approach to old ones, is a distinct service to any science.

# OBITUARY

## EDWARD WIGHT WASHBURN

DR. EDWARD WIGHT WASHBURN, chief of the division of chemistry of the National Bureau of Standards, died suddenly at his home from heart failure on February 6, at the age of 52 years.

Dr. Washburn was born at Beatrice, Nebraska, on May 10, 1881. He attended the University of Nebraska from 1899 to 1901 and graduated from the Massachusetts Institute of Technology in 1905. Here he continued his graduate work and received his doctorate in 1908.

Dr. Washburn's contributions to science include the writing of a text-book on physical chemistry, the editorship of the International Critical Tables, the publication of about 100 scientific papers and the direction and supervision of numerous researches. His professional career may be divided into five stages:

(1) As a graduate student at the Massachusetts Institute of Technology, 1905 to 1908, Washburn applied physico-chemical principles to analytical chemistry in the iodine-arsenious acid reaction, which resulted in the first thermodynamic treatment of the problem of "buffer" solutions, and later led him to a study of indicators. In this same period he made the first accurate measurements of true transference numbers and the relative hydration of ions in aqueous solutions of electrolytes.

(2) At the University of Illinois, 1908 to 1916, as a teacher and professor in physical chemistry, he produced his preeminent work in pure physical chemistry, including the thermodynamic treatment of the colligative properties of aqueous solutions; the development of a "simple system of thermodynamic chemistry" by means of his "perfect thermodynamic engine"; the measurement of Faraday's constant with the iodine coulometer, and the development of a high precision viscosimeter and of apparatus for the precise measurement of the electrical conductivity of aqueous solutions of electrolytes. In 1915 was published the first edition of his widely used text-book on "An Introduction to the Principles of Physical Chemistry." A second edition appeared in 1921, and a French translation was brought out in 1925.

(3) While head of the department of ceramic engineering at the University of Illinois, 1916 to 1922,

Washburn wrote, "Physical chemistry is coming to be not so much a definite branch or field of chemistry as it is an attitude or point of view and method of approach to problems in all branches of chemistry"; and it is not surprising that he applied the principles of physical chemistry and thermodynamics so effectively to this new field of ceramic chemistry. The work which was carried on in his laboratory during this period included the development of precision apparatus and technique for measuring the viscosity, density and surface tension of glasses at high temperatures, as well as studies on dissolved gases in glass and the theory and methods for measuring the porosity of ceramic substances.

(4) From 1922 to 1926, Washburn literally buried himself in the task of editing the International Critical Tables. Science and technology must forever owe a great debt to his perseverance, tenacity and sacrifice in carrying on this monumental undertaking.

(5) In 1926, he assumed the leadership of the division of chemistry at the Bureau of Standards. The beginning of this last period of his career saw his work on the International Critical Tables come to a successful end. Washburn now devoted himself to the application of the principles and methods of physical chemistry to the problems of chemistry and technology, and had leisure meanwhile to satisfy partially the innate scientific curiosity of his imaginative mind. In this period he initiated a program of thermochemical research having for its object the accurate determination of the thermochemical constants of substances important to science and industry; instituted and directed the extensive project of separating, identifying and determining the constituents of petroleum, which involved the development of many and varied types of new apparatus; directed the research on rubber hydrocarbons which resulted in obtaining the first rubber crystals; and found time to make many personal contributions to science, among which may be mentioned his exhaustive study of the "Standard States for Bomb Calorimetry." The crowning achievement of Washburn's scientific career came with his discovery in December, 1931, of the fractional electrolysis of water. This process has made possible the preparation of relatively large amounts of practically pure deuterium, or "heavy" hydrogen of atomic mass 2, and has resulted in the opening up of a new field of research in physics, chemistry and the biological sciences.

Washburn's appointment as chief of the division of chemistry infused new life and activity into the group. Quiet, friendly, yet withal a little reserved, his ability, fairness and dignity at once commanded admiration and respect, which soon ripened into lasting friendship.

Dr. Washburn was a member of the National Academy of Sciences, the American Chemical Society, the American Physical Society, and the American Ceramic Society, and he carried for years a tremendous burden of committee assignments. He served as chairman of the Division of Chemistry and Chemical Technology of the National Research Council, and was American commissioner of the Annual Tables of Physical and Chemical Constants. He was a member of the International Committee on Thermochemistry and chairman of the International Commission on Physico-Chemical Standards. He was on three occasions a delegate to the International Chemical Union, and active in the work of the International Research Council. When a serious illness in 1929 compelled him to relinquish such activities, Washburn did so with deep reluctance and regret.

In 1910, Dr. Washburn married Miss Sophie de Veer, of Boston, who died two years ago. Their four children survive them.

In Dr. Washburn's death, the Bureau of Standards has lost an outstanding member of its staff—a brilliant investigator, cut off in the zenith of his career.

LYMAN J. BRIGGS

## RECENT DEATHS

ARTHUR RANUM, professor of mathematics at Cornell University since 1923, died on February 28, in his sixty-fourth year.

DR. JAMES MUNSIE BELL, dean of the School of Applied Science in the University of North Carolina, a member of the faculty for the last twenty-four years, died on March 3 at the age of fifty-three years.

DR. THOMAS CLACHAR BROWN, teacher of geology at the high school at Fitchburg, Massachusetts, died on February 28. He was fifty-one years old.

THOMAS ERIC PEET, professor of Egyptology at the University of Oxford, died on February 22, in his fifty-second year.

SIR VINCENT RAVEN, a past president of the British Institution of Mechanical Engineers and formerly technical adviser to the London and North-Eastern Railway Company, died on February 14 at the age of seventy-five years.

# SCIENTIFIC EVENTS

## THE WORK OF THE GODMAN FUND

*The London Times* calls attention to the fact that January 15 was the centenary of the birth of Frederick Du Cane Godman, to whose memory the Godman Exploration Fund was inaugurated in 1920.

Godman and Osbert Salvin, a life-long friend,

adopted the idea of evolution, and as they followed out Darwin's theories in their collecting expeditions they conceived the idea of writing a Natural History of Central America. This became their life's work, so that it was thirty-six years before this monumental undertaking, comprising sixty-two volumes, was completed. For years collectors were kept in the field to furnish further material from every branch of natural science. Each group when completed was presented to the British Museum to be incorporated in the national collections—and the welfare of the museum, of which Godman became a trustee, remained to the end one of his greatest interests. Thus, after his death in 1919 the most fitting memorial appeared to be a fund to facilitate exploration for the benefit of the British Museum collections.

The Godman Exploration Fund was founded in 1920. At first the income of the fund was insufficient to enable the trustees to send out special expeditions but it was often used, with private donations, as a contribution towards the expenses of collectors already in the field.

The grants at first consisted of from £200 to £300, but later, as applications increased, it was found that even small sums of £10 to £20 enabled some valuable pieces of work to be undertaken or native collectors trained to carry on.

There have already been thirty expeditions ranging over the five continents, and the areas covered include six in Europe, confined to Great Britain and the Carpathians; seven in Asia, from Arabia and Siberia to Bhutan and Tonkin; seven in Africa, mainly in the neotropical regions, and five in South America, three in Australia and two in the West Indies. As the result of these expeditions more than 3,000 mammals have been added to the national collection, over 50 forms having been described as new to science.

The ornithological expeditions have resulted in the collection of over 6,000 skins, but their chief value lies in the fact that they have filled up many gaps both in our knowledge and collections of the birds of Southeastern Asia. In entomology many new species and even new genera have been collected, and Mr. Tam's expedition to the islands in the Gulf of Guinea has been of peculiar interest from the point of view of distribution. The field of botany has also been covered. Scientific work in geology has been furthered by the investigation of the fossil remains on the South Coast of England. The Eocene beds in Kent yielded interesting fossil remains of mammals, and from the Gower Caves were obtained bones and teeth of bear and cave lion.

The first important addition which more than doubled the capital of the Godman Exploration Fund was made by Mr. Oldfield-Thomas, who bequeathed the residue of his estate to the Godman trustees.

## THE ARBORETUM OF THE UNIVERSITY OF WISCONSIN

WITH the addition of two 20-acre tracts and 3,100 feet of shore line along the south side of Lake Wingra to the present area, the University of Wisconsin arboretum and wild life refuge now consists of nearly 500 acres of land.

Approval of the acquisition of the additional 40 acres of land and the shore line, which extends along the entire southern and southeastern side of the lake, has been given by the board of regents. Funds made available by the Tripp estate have made the establishment of the arboretum possible without the use of state funds.

Established more than a year ago, the arboretum gives to the university and to the state an excellent opportunity for experimentation in reforestation and propagation of wild life. Professors Aldo Leopold and William Longenecker are in charge of the project.

Reforestation and the propagation of wild life are both extremely important to Wisconsin, which must constantly look after its reputation as vacation ground and recreational center for the central states, it was pointed out. In line with this idea, plans for the development of the tract have been pushed forward rapidly during the past year.

More than 15,000 pine and spruce trees have been planted. The tract at present is the home of 12 species of game birds and 22 species of mammals. These are expected to be increased rapidly in the future, since the particular kinds of foods and cover needed by each species has been greatly improved.

A roadway has been built through the tract during the past year, and the construction of barracks and experimental laboratories are now under way.

The arboretum will not only provide the state with an experimental ground for investigations in forestry and wild life propagation, but will also be useful as a demonstration ground in teaching land owners of the state, especially farmers, the technique of conserving wild game and making marginal lands useful as hunting preserves.

Carrying out this idea, Professor Leopold will give this winter a course in game management. It is the first of its kind to be offered in any university in the country. Establishment of hunting grounds on marginal farm lands throughout the state is expected to be aided by this course. With well-stocked hunting grounds on their lands, farmers will be able to charge for the hunting privilege, and thus obtain a steady income from the marginal and otherwise unproductive lands on which they must pay taxes.

## THE AMERICAN CHEMICAL SOCIETY

THE report of the secretary and business manager of the American Chemical Society, Dr. Charles L. Parsons, giving an account of the activities of the society during the year 1933, has been published. Dr. Parsons writes in part:

In 1933 the American Chemical Society passed through a crisis and has apparently passed through it successfully. Owing to the reduction in salaries; to unemployment, and to the vagaries of foreign exchange, resignations increased from 753 in 1931 to 1,207 in 1932 and to 1,335 in 1933. Members dropped for delinquency increased from 521 in 1931 to 698 in 1932 and to 811 in 1933. Also new members elected decreased from 2,143 in 1931 to 1,456 in 1932 and to 1,264 in 1933. In addition, and most important of all, the subsidy of approximately $75,000 a year from the industry and the Chemical Foundation which the society had received, and which had been passed on to the membership in publications to the extent of approximately $5 *per capita*, was discontinued and the society was left without this important means of support. It was also very evident from hundreds of letters in connection with resignations that an increasing number of members objected strenuously to receiving all three journals when many of them desired only one or two. They felt they should not be called upon to the same extent in the matter of dues as those who received all three. The matter became so serious that a committee was appointed at the Washington meeting, which, through several months of careful study and argument by correspondence, finally met in New York with every member present for a conference lasting two days. Their report was presented at the Chicago meeting, was adopted unanimously by the directors and by an overwhelming majority by the council. Under this report two important departures were made in society procedure: first, the society was put on a strictly professional basis with definite professional requirements for all members entering the society after the adoption of the report; second, a new procedure was adopted by which the average cost to the member remained about the same as at present, but under which the deficit of 1933 will probably be eliminated by discontinuing the policy of requiring all members to pay for all three journals. The plan made it possible for those who wish to retain their membership in the society at a minimum cost to secure this privilege for only $9, including the *News Edition* which for the sake of society continuity, information and corroboration, it is felt necessary to send to every member. Under this procedure members may subscribe for one, two or three journals as they see fit and at prices less than one fourth of that for which any comparable journals can be obtained from any part of the world. All of this will be found in detail in the *News Edition* for September 20, 1933, and in the October *Proceedings* printed in the *Journal of the American Chemical Society*. The secretary is happy to report that the plan has been received favorably by a vast majority of the society, that subscriptions to each of the journals, both from members and from non-members, are far beyond the committee's calculations, and that the trend in membership loss has been stopped, for already on January 20, 807 new members have been received as compared with 1,264 for the full year of 1933, or approximately 300 more than had been received at the same period a year ago. In addition thereto, resignations between November 1 and January 20, which were 651 a year ago, have been reduced to 331, or almost exactly one half, in 1934. It is a gratification to be able to state that in twenty-six years of experience I have never known the membership as a whole more contented with any new procedure of the society than they are with the change that has been made. There are of course individuals who still do not approve, but that is always the case in any action which the council and the directors may take. It is unavoidable in a membership organization.

Through very strict economy, especially the reduction in the weight of the paper used in our journals, a careful study of its quality, reduction in printing costs, and every saving which the directors could make, the deficit which the directors budgeted for 1933 as $73,000 was reduced to approximately $45,000, which amount was taken from the society's reserves. The society's actual expenditures in 1933 were $444,362.32. They were $493,387.06 in 1932.

## THE EINSTEIN INSTITUTE OF PHYSICS IN JERUSALEM

IT was planned to dedicate on March 6 the Einstein Institute of Physics at the Hebrew University in Jerusalem.

According to the account in *The New York Times* those taking part in the ceremonies included Sir Arthur Grenfell Wauchope, High Commissioner of Palestine; Dr. Israel Wechsler, neurologist of Columbia University and member of the governing board of the Hebrew University; Dr. Chaim Weizmann, its president and former president of the World Zionist Organization, and Dr. Nelson Glueck, of the Hebrew Union College, Cincinnati, who now is in Palestine engaged in archeological work.

The building was begun in October, 1928, and cost about £15,000. It is "U" shaped. The central portion has two stories and a basement. The side wings are one story and are designed to permit the erection of additions.

The first laboratory in the new building will be devoted to spectrography. The instruments will be purchased through funds made available by the Elizabeth Thompson Science Fund of Boston, and a telescope has been donated by Dr. Leon Schoenfeld, of Zagreb, Jugoslavia.

A materials-testing laboratory, said to be the first of its kind in the Near East, also will be established in the new building. Its purpose will be to raise the standard of building construction in Palestine and neighboring countries.

During the dedication ceremony a plaque, bearing

the inscription "The Solomon and Dora Monness Shapiro Building of the Einstein Institute of Physics," will be unveiled in the presence of Mrs. Dora Monness Shapiro, the widow of Solomon Shapiro, of New York, in whose memory the building was donated.

The late Philip Wattenberg, also of New York, was joint contributor with Mr. and Mrs. Shapiro of the building. It is similar in construction to the Mathematics Building of the Einstein Institute of Mathematics on Mount Scopus, which was Mr. Wattenberg's gift.

# SCIENTIFIC NOTES AND NEWS

SIR JAMES HOPWOOD JEANS has been elected president of the British Association for the Advancement of Science in succession to the late Sir William Hardy. Sir James is now visiting the United States.

DEAN DEXTER S. KIMBALL, of the College of Engineering of Cornell University, was guest of honor at a recent dinner of the Engineering Society of the University of Pennsylvania. Dean Kimball delivered the address of the evening on "Old Features of the New Deal." Fifty educational and industrial leaders of Philadelphia attended the banquet.

AT the recent meeting in New York of the American Institute of Mining and Metallurgical Engineers, honorary membership was conferred on Albert Sauveur, Gordon McKay professor of metallurgy at Harvard University.

THE honorary degree of doctor of science will be conferred by the University of Dublin in July on Canon Victor Gregoire, professor of general biology and botany at the University of Louvain, and on Dr. D'Arcy Wentworth Thompson, professor of natural history at St. Andrews University.

SIR FREDERICK GOWLAND HOPKINS, professor of biochemistry at the University of Cambridge, president of the Royal Society; Professor G. H. Hardy, Sadleirian professor of pure mathematics at the University of Cambridge, and Dr. Erwin Schrödinger, formerly professor of theoretical physics at Berlin, now a fellow of Magdalen College, Oxford, have been elected honorary members of the Academy of Science at Leningrad.

OFFICERS of the Geological Society of London were elected at the annual meeting on February 16 as follows: *President,* J. F. N. Green; *Vice-presidents,* Professor P. G. H. Boswell, Professor W. S. Boulton, Sir Thomas H. Holland and Mr. W. Campbell Smith; *Secretaries,* Professor W. T. Gordon and L. Hawkes; *Foreign Secretary,* Sir Arthur Smith Woodward; *Treasurer,* Mr. F. N. Ashcroft.

AWARDS of advanced fellowships for study in Belgium under the auspices of the Commission for Relief in Belgium Educational Foundation, Incorporated, have been made for one year to Dr. Milton S. Plesset, National Research Council Fellow at the Institute for Theoretical Physics at Copenhagen, to study physics, especially quantum-electrodynamics, at the University of Liége, and to Dr. Jerome S. Smiser, instructor in geology at Princeton University, to study paleontology and stratigraphy at the Royal Museum of Natural History in Brussels. Fellowships awarded for a limited period have been granted to Edward G. Misner, professor of farm management at Cornell University, to study agricultural cooperation and credit in Belgium and to Charles B. Read, assistant geologist in the U. S. Geological Survey, to study paleobotany at the University of Liége.

DR. NEVIL MONROE HOPKINS, formerly of the George Washington University, will serve as acting professor of electrical engineering in the College of Engineering at New York University for the remainder of the year.

D. W. CHITTENDEN, assistant professor of animal husbandry and assistant animal husbandman of the University of Missouri, has accepted an appointment beginning on January 1 as chairman of the department of animal husbandry in Montana College.

AT Yale University, Dr. Cornelius B. Osgood has been promoted to be curator of anthropology in the Peabody Museum of Natural History, and Dr. Hugh M. Wilson has been appointed assistant professor of radiology.

DAVID BRUNT, since 1919 superintendent of the British Army Services Division of the Meteorological Office, has been appointed to the university chair of meteorology at the Royal College of Science, University of London.

DR. J. SANCHEZ COVISA, formerly president of the College of Physicians of Madrid, has been appointed dean of the medical faculty of the University of Madrid to succeed the late Dr. Sebastian Recasens Girol.

IT is reported that Dr. Rexford Guy Tugwell, assistant secretary of agriculture, will be placed at the head of a commission to Puerto Rico to study conditions there in an effort to raise the standards of living. The survey will be made in collaboration with Governor Winship.

PAUL G. REDINGTON, chief of the Bureau of Biological Survey for the past seven years and for

twenty-three years with the Forest Service, returned on March 1 to the service. This transfer is brought about at his own request. W. C. Henderson, associate chief, will be acting chief of the Bureau of Biological Survey. Secretary Wallace, in announcing the transfer, expressed his appreciation of Mr. Redington's services with the Biological Survey.

B. Y. MORRISON, for the past six years assistant chief of the Bureau of Plant Industry of the U. S. Department of Agriculture, has been appointed chief of plant introduction. Mr. Morrison succeeds Knowles A. Ryerson, who became chief of the bureau on January 1. Dr. M. A. McCall, in charge of the Division of Cereal Crops and Diseases, in addition has been placed in charge of cotton and other fiber investigations.

DR. HOWARD R. TOLLEY, director of the Giannini Foundation of Agricultural Economics of the University of California, has been granted an extension of his leave of absence to June 30 to enable him to continue as chief economist in the Agricultural Adjustment Administration in the U. S. Department of Agriculture. Leave of absence has also been granted to Dr. M. R. Benedict, professor of agricultural economics and agricultural economist, in order that he may take part in a study of the statistical work of several departments of the Federal Government. Dr. Harry R. Wellman, extension specialist in agricultural economics, has been requested to serve as chief of the specialty crops section of the Agricultural Adjustment Administration.

DON S. ANDERSON, of the department of economics of the College of Agriculture of the University of Wisconsin, has been called to Washington, D. C., where he will assist with the dairy adjustment plans of the Agricultural Adjustment Administration. It is expected that the work will occupy approximately three months.

PROFESSOR R. ADAMS DUTCHER, head of the department of agricultural and biological chemistry of the Pennsylvania State College, sailed for Germany on March 1. For the next six months he expects to devote the major portion of his time to visiting the universities and experiment stations of that country. He is especially interested in making a detailed survey of the vitamin research laboratories in ten other northern and central European countries, in addition to those of Germany. On March 28 he will attend the Technical and Chemical International Congress of Agricultural Industries at Paris. During his trip abroad Professor Dutcher is serving as a collaborator for the Federal Bureau of Chemistry and Soils and as a fellow of the Oberlaender Trust of the Carl Schurz Memorial Foundation.

DR. H. H. LOVE, who is on leave of absence from Cornell University to serve as agricultural adviser to the Ministry of Industries of the Government of China, has been working with some Chinese agriculturists for the organization of a National Agricultural Research Bureau for China. Such a bureau is now established, and a number of investigators are already at work on a number of important agricultural projects. Land has been bought, and a new building for investigational work is already under way. This is the first important step taken by the new National Government to promote the agricultural industry of China. The purpose of the bureau is to develop work along those lines that are of national importance, leaving the agricultural problems of local importance for the provincial governments to handle. Dr. Love is now serving as technical adviser to the bureau and as chief of the Division of Crop Improvement.

PROFESSOR P. W. BRIDGMAN, of Harvard University, was guest speaker at the initiation ceremonies of the Sigma Xi at the University of Pittsburgh, on February 14. His topic was "The Behavior of Material under High Pressures." On the previous day he spoke before the Pittsburgh Physical Society, on "Are Our Conventional Electrical Concepts Adequate for Thermoelectric Phenomena?"

DR. EDWIN B. WILSON, professor of vital statistics, School of Public Health, Harvard University, delivered the principal address in connection with the annual Sigma Xi Day held on February 22 at the University of Rochester. Dr. Wilson's topic was "Factors in Mental Ability." The program also included a science lecture for young people, given by Dr. Sherman C. Bishop, professor of zoology at the University of Rochester, and a series of six lecture-demonstrations of current scientific research by members of the Rochester Chapter.

DR. JAMES FRANCK, Nobel laureate, formerly head of the second Physical Institute, University of Göttingen, lectured on "Catalysis" on March 3, before a joint meeting of the Washington Academy of Sciences and the Philosophical Society of Washington.

PROFESSOR LOUIS KAHLENBERG addressed the Akron Section of the American Chemical Society on February 15 on "The Relationship between Electrical Potentials and Chemical Reactivity." On the following day he spoke to the Columbus Section at the Ohio State University.

THE eighth annual Priestley Lectures at the Pennsylvania State College will be given at 7:30 each evening from March 19 to 23 in the Chemistry Amphitheater. This series deals with the border line between physical chemistry and some other branch of science. Dr. Ross Aiken Gortner, professor of agricultural biochemistry at the University of Minne-

sota, will give the lectures this year. His subject is "The Borderline between Physical Chemistry and Life Processes." He will treat in these lectures "The Colloidal State of Matter"; "Electrical Forces at the Interface," and "Water in the Colloidal State and the Rôle of 'Bound Water' in Living Processes." The Priestley Lectures were inaugurated by the faculty of the college in 1926. In 1931, Phi Lambda Upsilon (honorary chemical fraternity) undertook their financial support.

ARRANGEMENTS have been made for the next annual meeting of the American Association of Physical Anthropologists to be held at the American Museum of Natural History in New York, from May 8 to 10, inclusive. The time of the meeting will be concurrent with that of the American Society of Mammalogists (May 8 to 12). A joint session with that society is planned for the morning of May 10 and will be devoted chiefly to papers on primates.

THE eighty-second annual meeting of the American Pharmaceutical Association and affiliated organizations, during which the American Institute of Pharmacy will be dedicated, will be held at Washington from May 7 to 12. Members of the District of Columbia Pharmaceutical Association will act as hosts. The general committee of arrangements, headed by President Paul Pearson, of the District Association, is completing arrangements for the business and entertainment features of the program.

THE Chicago Chapter of the Rocks and Minerals Association, an organization for citizens of Chicago who take interest in geology and mineralogy, held its initial meeting on February 17, with thirty-four persons present. The group met at the Museum of Science and Industry for a tour of the museum, including the full-sized working coal mine which is being shown there. The excursion was followed by a "get-acquainted" dinner at the Broadview Hotel, at which Dr. D. J. Fisher, of the University of Chicago, spoke informally on "Collecting Rocks and Minerals Around Chicago," and Mr. F. L. Fleener, of the Joliet Township Junior College, spoke on the Wilmington strip mines as a collecting ground for fossil-bearing nodules. Mr. Ben H. Wilson, national director of program building and research of the Rocks and Minerals Association, explained to the group the significance and purpose of the association and made various suggestions for the future activities of the Chicago Chapter, of which G. Frederick Shepherd is chairman.

ACCORDING to *Industrial and Engineering Chemistry,* to mark the twenty-first anniversary of the South African Chemical Institute, there has been issued a pamphlet containing the addresses given on the occasion, the meetings being held in Johannesburg in May, 1933. In addition to an address broadcast by J. A. Wilkinson and remarks by the deputy mayor of Johannesburg and the president of the Transvaal Chamber of Mines, a series of papers was presented on the history of the institute and the relation of chemistry to the community, to industry, to agriculture, to medicine, to fuels and to the coal industry. Another paper discusses chemistry in the extraction of base metals.

*The Experiment Station Record* reports that the Frasch Chemical Foundation at the University of Missouri has approved an extension for a second five years of the station project on growth and development of domestic animals. The new grant will provide $9,000 per year from this source, and the work will be continued under the immediate direction of Dr. S. Brody and the supervision of a special station committee, consisting of the director and the chairmen of the departments of agricultural chemistry, animal husbandry, dairy husbandry and poultry husbandry.

THE Bernice Pauahi Bishop Museum, Honolulu, T. H., has recently donated to the Yale School of Forestry over 2,500 specimens of wood from the islands of the South Seas. This is the largest single addition ever made to the Yale collections and brings the number of catalogued samples up to nearly 26,600. The object of the gift, according to Professor Samuel J. Record, is to further a comprehensive systematic study of the woods of the entire world which is being sponsored by Yale University in cooperation with the International Association of Wood Anatomists. The Bishop Museum specimens are said to be of exceptional value because nearly all were obtained through expeditions to remote localities in the Pacific, for example, Fiji and Samoa, and Austral, Cook, Hoorn, Lauai, Line, Marquesas, Maui, Phoenix, Rapa, Raratea, Society, Tubuai and Wake Islands.

ACCORDING to a summary given by a correspondent of the London *Times,* changes in the constitution of the British Association of Chemists were made at the sixteenth annual meeting of members recently held in Manchester. Ordinary members of the association contribute compulsorily to an unemployment fund. It is now proposed to establish a new class. The council shall have power at their discretion to elect as fellows eminent persons interested in the objects of the association. In the discussion it was clear that the proposed new class of members would have no power of voting on the disposal of the unemployment fund and their subscriptions would not be applied to it. The name "fellow," however, was felt to be not quite satisfactory, and as there was equal difficulty about calling them "honorary" members, since they would be expected to contribute, the meeting finally passed the

resolution amended so as to leave the exact definition to be supplied by the council later. Another change, also strongly challenged before being adopted, was that giving the council "power at their discretion to elect as associates chemists not of British nationality who would otherwise be qualified for full membership." This was objected to as if it were against the economic interests of British chemists, but the president, Professor E. C. C. Baly, according to the correspondent, made it perfectly clear that the intention was simply to enable that association to be courteous and friendly to the alien chemists who are admitted to England by the Ministry of Labor under special license for limited periods and for particular jobs. The proposed new rule was therefore amended so as to limit such associateship of alien members to the period of their stay. The new rules came into force on December 1.

# DISCUSSION

## SYMBOLS AND NAMES FOR THE HYDROGEN ISOTOPES

RECENT discussion[1,2] concerning names and symbols for the hydrogen isotopes illustrates a typical difficulty in scientific nomenclature. The problem continually arises of choosing or compromising between conflicting ideals: (1) maximum brevity, simplicity and euphony in important individual names; (2) maximum explicitness, completeness and consistency of characterization of individuals or individual types as members of a class or system; (3) the principle that well-established nomenclature, even if not particularly good, should not be too hastily set aside; but if, for sufficient reasons, it is replaced, the new nomenclature should not contain old names or symbols used with new meanings. In connection with (1), there is a tendency to prefer names, which give a feeling of qualitative individuality, to numbers; while with (2), there is a tendency toward complex symbols, often including numbers.

The use of different names for different isotopes of an element is natural when the isotopes have appreciably distinct individualities. Among radioactive elements, such individual names are very natural from the standpoint of radioactive family relationships, although uncalled for from a purely chemical view-point. The assignment of names to the hydrogen isotopes is based on the expected existence of appreciable differences in chemical behavior, together with the not irrelevant fact that the heavy isotope can be isolated in quantity.[1]

Logically, we might name isotopes like plants and animals. We speak of *Quercus alba* and *Q. rubra;* why not *Hydrogen protium* and *Hydrogen deuterium* as complete systematic names for the hydrogen isotopes? In practise, of course, the complete names would seldom be used, but instead usually just the genus or just the species name. Or one might adopt as standard nomenclature the generic name hydrogen and the individual names hydroprotium and hydrodeuterium, the latter abbreviated ordinarily to the terms protium and deuterium proposed by Urey, Murphy and Brickwedde.[1]

Turning to symbols, we find $H^1$, $H^2$, $Cl^{35}$, $Cl^{37}$, and so on, in common use. These, however, especially $H^2$ because the small number two occurs often for another purpose, as in $H^2{}_2O$, tend to be somewhat confusing and hard to read. Moreover, they make extra work in typewriting and typesetting and, especially with the hydrogen isotopes, are liable to confusing misprints, *e.g.*, $H_2{}^2O$, $H_2{}^1$. The form $(H^2)_2O$ is clear but too cumbersome. $H^2_2O$ involves difficulties and expense in typesetting. Most of the troubles just mentioned could be avoided by using H1, H2, Cl35; $H1_2O$, H1H2O, $H2_2O$; $H1_2O16$, H1H2017; $Cl35_2$; $C_6H1_3H2_3$; and so on. With a little practise, H1, H2 or O16 is recognized as a single symbol, just as are the two letters in Cl.

Whatever practise may be followed for isotopes in general, it seems clear that, at least for hydrogen, new symbols must be adopted. As Professor Urey has pointed out in correspondence, even H1, H2 will very likely not do, because of confusion sometimes arising when these are spoken as parts of complex formulas.[1] Some investigators[3] now use H and D for $H^1$ and $H^2$. This practise is open to serious objection in that it provides no means of distinguishing between the genus hydrogen (H) and the species *H. protium* (here also H). Usually, to be sure, there may be no danger of confusion, but it seems likely that in the future we shall wish to speak sometimes of the properties of, for example, $H_2O$, other times of the (more sharply defined) properties of $H1_2O16$, so that it would be far wiser to introduce a new symbol for protium, leaving H for the genus hydrogen (*cf.* ideal 3).

Unfortunately, we can not use P for protium. Pm might be used, or perhaps M or Z. We could then speak of $H_2O$, $Z_2O$, $D_2O$. The use of Z and D makes maximum concessions to ideal (1), but at the expense of ideal (2), since Z and D look like symbols for

[1] Urey, Murphy and Brickwedde, *Jour. Chem. Physics*, 1, 513, 1933.

[2] R. W. Wood, SCIENCE, 78, 532, 1933; Urey, Brickwedde and Murphy, and F. C. Whitmore, *ibid*, 78, 602–3, 1933; and later discussion in SCIENCE and *Nature*.

[3] *Cf., e.g.*, H. C. Taylor and J. C. Jungers, *Jour. Am. Chem. Soc.*, 55, 5057, 1933.

new elements. This objection would be much weakened by using Hp, Hd for *H. protium, H. deuterium.* It could be entirely overcome by using $H^p$, $H^d$, or better by the simpler symbols Hπ, Hδ. These would at the same time make a clear and obvious distinction between the genus or element symbol H and the species or isotope symbols π, δ. Still simpler would be Π, Δ. Their use would not seriously violate ideal (2), since the Greek letters would make it clear that we were dealing with isotope, not element symbols. Π and Δ would be easy to pronounce, and would ordinarily add little or nothing to the cost of typesetting, either in ordinary or in structural formulas.

Returning to nomenclature, one wonders whether such names as ammonia, methane, benzene should refer to $N\Pi_3$, $C\Pi_4$, $C_6\Pi_6$, or to $NH_3$, $CH_4$, $C_6H_6$. This is of importance because we expect soon to be able to experiment rather freely with mixtures of varied isotopic composition. It would seem logical (ideal 2) to use "benzene" as a generic name for all the isotopes and their mixtures. On the other hand, a simpler nomenclature (ideal 1) can be developed if we regard $C_6Hp_6$ as the orthodox benzene, from which isotopes can be obtained by various substitutions of Hd for Hp.

It seems likely that the two view-points may be combined in a practicable way somewhat as follows. The terms benzene, ammonia, *m*-dinitrobenzene, etc., will be used in a generic sense, covering the ordinary present-day materials as well as other isotopic mixtures. At the same time a more specific nomenclature, somewhat as follows, will be used in cases where the isotopic composition is known and is important: $N\Pi_3$, protiumammonia; $N\Pi_2\Delta$, deuteroprotiumammonia; $N\Pi\Delta_2$, dideuteroprotiumammonia; $N\Delta_3$, trideuteroprotiumammonia. In order to avoid constant repetition of the word protium, the brief letter Π or π could be used instead: Π-ammonia, deuteroΠ-ammonia, etc. Complete omission of the word protium or Π, while allowable if the context leaves no doubt as to what is meant, would in some cases lead to confusion or at least make understanding more difficult, and should therefore not be indulged in too freely.

Just as logical as the preceding would be the scheme $N\Delta_3$, deuteriumammonia; $N\Delta_2\Pi$, protodeuteriumammonia, and so on (abbreviations Δ-ammonia, protoΔ-ammonia, etc.). In these schemes, there seems to be no danger of confusion between the prefixes protium- and proto-, or deuterium- and deutero-, if each is used consistently in a definite way. It is entirely natural to use proto-, deutero-, like nitro- in nitrobenzene, in order to refer to substitutions made in an initial or standard substance. The nouns protium, deuterium, on the other hand, are used descriptively in much the same way as "house" or "sheep" in houseboat or sheep-ranch. Protiumbenzene means all-protium benzene (no deuterium) just as sheep-ranch, strictly interpreted, means all sheep (no cattle).

Both the above schemes might be used interchangeably, but it would probably be wiser to keep to a single set of standard names. Preference would naturally be given to the first scheme, except that perhaps one might adopt deuteriumammonia, etc., from the second, for the pure Δ compounds.[4]

Thus we should have: Π-ammonia (*i.e.*, protiumammonia), deuteroΠ-ammonia; Π-benzene, deuteroΠ-benzene, *o*-, *m*- and *p*-dideuteroΠ-benzene, . . . hexadeuteroΠ-benzene or deuteriumbenzene (Δ-benzene). Further: chloroΠ-benzene, *m*-deuterochloroΠ-benzene; Π-methyl chloride, deuteroΠ-methyl chloride: *cis*-dideuteroΠ-ethylene; Π-chloroform, deuteroΠ-chloroform or Δ-chloroform. The use of Δ- as a prefix may be open to some objection because δ- is frequently used for another purpose in organic chemistry.

It has been proposed[5] that "deuton" be replaced by "diplon" for the $H^2$-particle, and deuterium by diplogen for the $H^2$ atomic species. Although a close relation between particle and species names (*cf.* proton and protium) has advantages, it does not seem essential, especially since the names are used in quite different fields of research. Moreover, strong objections exist to diplogen, since its ending could suggest oxygen or nitrogen just as well as hydrogen. The set of names hydrogen (H), protium (Π), diplogen (Δ) is recommended by no good systematic argument. Better would be hydrogen, (hydro)protium, (hydro)diplium. There seems, however, to be no sufficient reason for giving up "deuterium" even if deuton is replaced by diplon, H2-particle, or some other name.[6]

ROBERT S. MULLIKEN

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## TALUS SLOPES OF THE GASPE PENINSULA

THE Syracuse University Museum of Natural Science maintained a group of scientists in the field during the months of July and August. The members carried on collecting activities for the museum in the various branches of natural science and pursued scien-

[4] Another scheme would be to use hexaprotiumbenzene, deuteriumpentaprotiumbenzene, and so on, but it seems unnecessary to go to such lengths for the sake of ideal 2. In some cases, *e.g.*, diprotiumwater, protiumdeuteriumwater, diprotium, this scheme would be fairly simple, but it would nevertheless make less confusion if it were avoided, especially since the alternatives (Π-water, deutero Π-water, Π-hydrogen) are also simple.

[5] *Cf.* SCIENCE, 79, 26, 1934; Lord Rutherford, *Nature*, 132, 955 (1933).

[6] A simple although meaningless name would be delton; the corresponding species name (hydro)deltium would also be not unpleasant.

tific studies in their respective fields. The area selected for field study was the Gaspé Peninsula in the northeastern portion of the province of Quebec, Canada. It was my privilege to carry on a part of the geological studies in conjunction with Dr. E. T. Apfel, of the geology staff of Syracuse University, and curator of geology in the Syracuse University Museum of Natural Science.

The talus slopes of the northern coast of the peninsula presented an interesting study. The angle of rest of coarse materials does not usually exceed 35 degrees. However, the slopes along the north coast of the Gaspé Peninsula exceeded this dip generally.

The best-developed slopes were in the section of the northern coast between the villages of Mont St. Pierre and Rivière Madeliene. Through this section, the towering cliffs of the Ordovician limestones, shale and sandstones, attaining altitudes of more than 300 feet, rise abruptly from the highway bordering the St. Lawrence River. The talus slopes at the base of these cliffs have angles of slope from 36 to 40 degrees. The lesser angle was only encountered once, all other slopes ranged in dip from 38 to 40 degrees.

The material comprising the slopes was for the most part made up of angular, elongated calcareous slate fragments. They were sufficiently stable so that the slopes could be walked upon, with but little disturbance of the material comprising them.

At Cap Gros Morue, fifteen miles west of Rivière Madeliene, the slope assumed an angle of repose of 36 degrees. This particular slope was limited to about 20 yards along the road. Immediately adjoining this part, the slope increased to 38 degrees. In both instances the height of the slope was approximately 70 feet. A hundred yards east, the slope displayed had an angle of 40 degrees and a height of 150 feet. This particular slope was exceedingly firm. Walking across its surface, very little material was disturbed, other than that on the immediate surface. The talus consisted in the main of particles of calcareous shale with slate, ranging in size from ½ inch to 5 inches in length with the intermediate sizes, 1 to 3 inches, predominating.

Two hundred yards west of Cap Gros Morue, the slope measured 39 degrees and the same characteristics prevailed as at those noted farther to the east. About 200 yards west of Mont St. Pierre, a talus slope borders the road for 400 yards and rises 175 feet above it. The angle of rest here was 39 degrees. Readings were taken at several points over the surface of the slope and carefully checked.

Other slopes were studied along the face of the cliffs on the northern coast and all were found to have angles of repose of 38 to 39 degrees, including the smaller slopes. The locations of the smaller slopes are not given here, as the most significant portion of the northern coast, in as far as the talus slopes are concerned, has been described.

It seems singular that an area in which so many talus slopes occur should contain so many with an unusually high angle of rest; also the absence of slides in an area of this nature commands attention.

A close examination of the slopes disclosed the fact that each fragment, resting with its long axes parallel to the dip of the slope, was overlapped by the preceding one forming an end drag on each piece. The arrangement was so orderly that, viewed as a whole, it gave the appearance of having been laid by hand. The enechelon arrangement of the fragments and the elongated manner in which the rock weathers apparently accounts for the compactness and rigidity of the slopes.

NEIL A. MINER

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## FURTHER EVIDENCE ON THE AMAZING LONGEVITY OF BACTERIA

IN 1931[1] I published an account of experiments which demonstrated the existence of living bacteria in anthracite coal from Wales and from Pennsylvania. Shortly thereafter[2] there appeared an article by the distinguished German bacteriologist, R. Lieske, in which results of similar studies by him (antedating mine but unknown to me when my work was in progress) give complete concordance with my results and important supplementary data besides. Lieske and I differ only in regard to the explanation of how the bacteria have survived in the coal for untold ages. He believes that the coal bacteria carry on a weakly vegetative existence there, whereas I believe that they exist in a resting stage in the coal and either do not respire at all or only with extreme slowness. I still believe that my hypothesis is correct. But the purpose of this note is not to discuss hypotheses which I shall treat more adequately elsewhere, but to call attention to a great mass of additional experimental data which I have obtained by another method of attack on the problem of longevity of bacteria. Since it may be some time before my extensive data on these newer studies can be published I give a few of the results herewith.

One of the objections made to my work on anthracite coal and on other ancient rocks is that the small numbers of bacteria which I found in such materials always suggest the question of possible invaders or contaminants. I have, I think, shown this criticism to be invalid, but to strengthen my position and render more plausible my contention respecting the

[1] *Jour. Bact.*, 22: 3, September, 1931.
[2] *Biochem. Zeitsch.*, 250: 1–6, July, 1932.

longevity of bacterial spores or other resting stages of bacteria I have been carrying on studies of historic and prehistoric materials whose age is reasonably well known and in which large numbers of bacteria might be expected. I started with soils which had been kept in sealed bottles for 25 and 33 years, respectively, and then continued with subsoils which had been in bottles unopened for 65 years. In all these samples living bacteria are very numerous and in great variety and all the soils contained either single-celled green or blue-green algae or both in vigorous condition. I then proceeded to a study of adobe bricks taken from the interior of thick walls in the California missions from structures 112 to 150 years old. All these contained enormous numbers of living bacteria, and the youngest of them also showed single-celled green algae. Following these materials, I turned to some of the Arizona pueblos whose age is definitely known to be no less than 600 years old. From material in the heart of the pueblo walls I isolated many forms of bacteria, and the total numbers, while smaller than those in the mission bricks, are still very high. I proceeded next to examine respectively adobe bricks from pre-Inca pyramids near Lima, Peru, specially collected for me from the interior of the pyramid, and adobe bricks from pre-Aztec pyramids in Mexico, also collected expressly for me under special instructions. The age of the former is estimated by archeologists to be between 1,000 and 1,400 years and that of the latter no less than 800 to 1,000 years. In both of these adobe materials bacteria, while not as plentiful as in fresh soil, are still very numerous and comprise a great variety of forms which grow on "selective" media. Space is too limited in this note to permit of giving the detailed technique employed in these experiments, which will be described elsewhere, but attention is called to the remarkable longevity of both bacteria and of simple algae as they appear, respectively, in these experiments. I may add also that some of the mission walls have been protected from water during all their history, and all my specimens, whether from protected walls or not, are found to be extremely desiccated for the reason that they come from the heart of the wall to which water does not penetrate, as examinations at the end of the rainy season clearly attest.

Many still older materials than those described will be investigated and reported upon later.

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## A POSSIBLE CAUSE OF OLD AGE

HEAVY water has a higher boiling point (101.42° C.) than ordinary water. Heavy water also inhibits the growth of seedlings, which seems to indicate that it has an inhibitory effect upon the normal functioning of the protoplasm. As the human body evaporates a large proportion of its water intake, it will in the course of years become enriched with heavy water. This increase in the proportion of heavy water in the body fluids may account for the increasing inhibitory action of the protoplasm during senility.

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# REPORTS

## RESEARCH IN THE YALE GRADUATE SCHOOL

SEVENTY-FOUR research projects have been undertaken by students already holding the degree of doctor of philosophy or its equivalent, who are enrolled in the Yale Graduate School as research fellows. Nine countries are represented in this group, including England, Germany, Canada, Belgium, China, Czechoslovakia, New Zealand, Norway and the United States. These men and women have been trained for their research at sixty different colleges and universities, and while most of them are working either in the Sterling Memorial Library or the laboratories of the university, some are working in places so remote as the Bishop Museum in Honolulu, the Yale Anthropoid Experiment Station in Orange Park, Fla., the Navajo and Shawnee Indian Reservations and the British Museum.

In addition to the faculty, library and laboratory facilities put at the disposal of research workers, a number of research fellowships to assist this group of scholars have been established. This year forty-one research fellowships with stipends were awarded. In addition, twenty honorary research fellows have been appointed without stipend. Another group of fellows has been sent to work at Yale by educational foundations, including the National Research Council, the Rockefeller Foundation, the American Council of Learned Societies, the Commission for Relief in Belgium, the Commonwealth Fund and the Alumni Association of former German Exchange Fellows.

Fifty-two of the seventy-four fellows are carrying on scientific work, while twenty-two are studying languages, philosophy and the social sciences. Chemistry has the largest group with seventeen working at the Sterling Chemistry Laboratory and five in the lab-

oratory of the Department of Physiological Chemistry.

Richard J. Block, of Scarsdale, N. Y., is carrying on a study of brain proteins in the laboratory of physiological chemistry. Robert O. Bengis, of New Haven, Conn., is studying the chemistry of coffee at the Sterling Chemistry Laboratory; Ebbe C. Hoff, of Lindsborg, Kans., is working on the finer structure of the nervous system and its correlation with nervous physiology, and Helen G. Richter, of New Haven, is making a pathological study of nervous tissues in dogs with a diet deficient in vitamin.

Sterling fellows are investigating the following subjects: Ernest Beaglehole, Wellington, New Zealand, cultural stratification in Polynesia; Robert M. Brick, Ventnor, N. J., at the Hammond Metallurgical Laboratories, grain boundaries as variables in solid equilibria relationships; John M. Bruhn, of Sioux Falls, South Dakota, at the Yale Anthropoid Experiment Station, the relation of basal metabolic rate to lesions in the cerebral cortex; Charles Burnham, of Fort Atkinson, Wis., meiosis in maize plants, and John R. Huffman, of West Haven, Conn., unimolecular homogeneous gas reactions.

Cecil T. Lane, of Montreal, is studying magnetism at the Sloane Physics Laboratory; Saunders MacLane, of Norwalk, Conn., is working in the mathematics department on the topology of algebraic systems, and Normal D. Newell, New Haven, Conn., is working at the Peabody Museum on a study of the late Paleozoic Pelecypode, of the United States.

The theory of electrolytes is being investigated in the Sterling Chemistry Laboratory by Lars Onsager, a Norwegian. Ernest C. Pollard, of Lincolnshire, England, is studying nuclear physics at the Sloane Physics Laboratory.

Julien A. Ripley, Jr., of Oyster Bay, N. Y., is applying scientific methods of research in the social sciences, particularly sociology; Daniel Raffel, of New Haven, Conn., is working at the Osborn Zoological Laboratory on the translocutions of parts of chromosomes of *Drosophila melanogaster* produced by x-rays; Otto L. Tinklepaugh, of New York City, is studying the relation of maturational factors to the social behavior of the chimpanzee and the macaque monkey; Harold Henderson Williams, of Howard, Pa., is studying the possible rôle of cholesterol in fat metabolism.

As Currier fellow in history, Bell I. Wiley, of Halls, Tenn., is collecting material in the South for a study of the Negro in the Confederacy. The new Indiana Fellowship in Anthropology is held by Charles F. Voegelin, of Berkeley, Calif., who is working in the field collecting material among the Shawnee Indians of Oklahoma. Two Bishop Museum fellows are working in the Islands of the Pacific. Albert C. Smith, of New York City, is making extensive botanical collections in a group of Polynesian Islands, and Laura M. Thompson, of Berkeley, Calif., is making an intensive ethnographical survey of the culture of one island in the Lau group.

## REPORT OF THE TRUSTEES OF THE BANTING RESEARCH FOUNDATION

THE Banting Research Foundation has now been in active operation for six and a half years, though in the first two years the full capital sum was not available and the number of grants made were few. The capital sum now amounts to about $700,000 and the number of individual grantees has steadily increased. The total number of grants made during the period is ninety-two. These have been distributed to 63 workers in the following universities: Alberta 4, Saskatchewan 2, Dalhousie 8, Queen's 2, Western Ontario 2, Manitoba 16, McGill 26, Toronto 30 and 2 non-university. Some 50 papers have already appeared in scientific publications, while some 15 papers are in press or ready for publication. Several pieces of work are not as yet complete.

In accordance with its charter, the foundation also aids in the support of the Department of Medical Research (Dr. F. G. Banting) and from this source numerous papers have appeared, including ones dealing with the problem of silicosis and of the action of vitamins.

Important papers on the following subjects have been published during the past year by grantees: "On the Functioning of the Thyroid Gland" (A. C. Abbott, Manitoba), "Gastric Secretion" (A. M. Alley, McGill), "Anterior Poliomyelitis" (M. Brodie, McGill), "Addison's Disease" (M. M. Cantor, Alberta), "Fungous Diseases of the Skin" (A. M. Davidson, Manitoba), "Glycogen Metabolism" (G. T. Evans, McGill), "Thorium Oxide and the Reticulo-endothelium" (R. Gottlieb, McGill), "Urate Excretion" (H. C. Graham with E. G. Young, Dalhousie), "The Effect of Choline on Fat Metabolism" (J. M. Hershey and C. H. Best, Toronto), "The Fate of Lactic Acid in the Body" (F. L. Horsfall, Jr., McGill), "On the Function of the Lachrymal Gland" (P. R. McDonald, McGill), "A Study of the Histology of the Human Ovary" (D. Mainland, Dalhousie), "On Obstetrical Forceps" (J. Mann, Toronto), "Cholesterol Metabolism" (J. M. McEachren, Manitoba), "Urinary Antiseptics" (D. R. Mitchell and J. M. Scott, Toronto), "Action of Drugs on the Uterus" (R. A. Moreash and N. B. Dreyer, Dalhousie), "Serum Bilirubin" (F. D. White, Manitoba).

During the past year twenty workers received grants from this fund. The trustees feel assured that the

work done under the aegis of the foundation has been of real value and that the foundation, which is the only one in Canada lending its support to medical research, has proved a valuable aid and stimulus to such research in Canada. Like all such foundations, the depression has increased the demands upon it and a larger revenue could be expended with advantage.

VELYIEN E. HENDERSON
D. T. FRASER
*Honorary Secretaries*

# SCIENTIFIC APPARATUS AND LABORATORY METHODS

## A MICRO-MANIPULATOR FOR PURE CULTURE AND MICROCHEMICAL WORK

THE rapid selection of unicellular organisms for pure culture presents problems which differ so markedly from those involved in cell dissection, injection and aspiration that they require not only a different technique but, if a high ratio of success is to be achieved, changes from existing standards in the form and arrangement of parts of the manipulator itself.

To adapt my recently introduced micro-manipulator[1] to this line of work, I have made a number of changes in the standard form. The new instrument has proved in use not to be limited to the field of biology but to meet the demands of microchemical work. By the use of the pipette point as a test tube, it permits the rapid chemical analyses of particles of the order of $10^{-7}$ gm, and is therefore available for the convenient analysis of even the most valuable works of art.

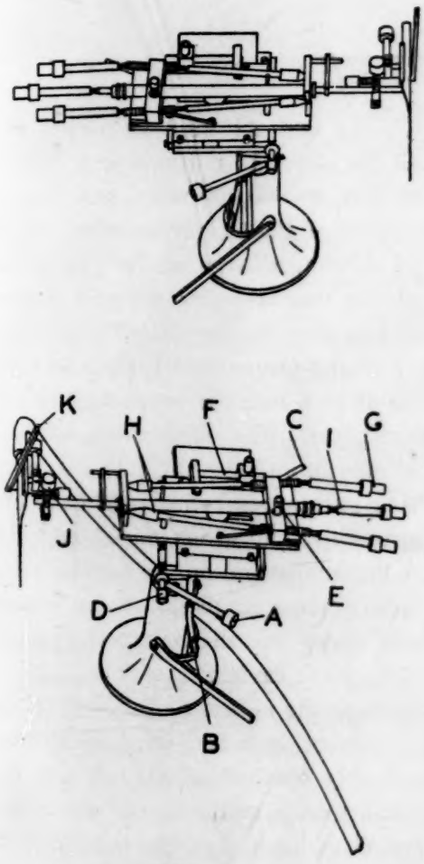


FIG. 1

The instruments as illustrated are arranged for the pipettes to enter the moist chamber from the front and to converge at the optical axis of the microscope. The pipettes are held at right angles to the MM shaft, that the grouped MM controls may be within instant grasp by the operator. When desired, the pipettes may be held parallel to the MM shaft, in which case the manipulators may be grouped on one side of the microscope.

The manipulator is supported by a pedestal and the vertical gross adjustment occurs within the hollow post of the pedestal through the up-and-down movement of the vertical stem of the supporting bracket. The bracket is set to the height of the microscope stage and its stem is grasped by a collar clamped by the long-stemmed screw A. The superimposed apparatus is raised and lowered through a range of one half inch by means of the lever B, which has a power ratio of 4 to 1. A coiled spring within the bracket stem supports the weight and permits a uniform control through the smooth resistance of polished metal on the graphite-lubricated felt friction rings of the bracket stem. The resistance is sufficient to hold any given position securely, yet instant adjustment may be made with smoothness and accuracy.

The shelf of the bracket supports a plate which glides between felt-lined guides attached to the bracket shelf, in the axial line of the pipette. It is actuated by lever C with a power ratio of 3 to 1 and has a range of 2 inches, that clearance may be given for the safe installation of the pipette. An adjustable stop D is attached to the shelf of the bracket and can be set when the point of the pipette is in the center of the field. When the pipette is moved away to install the moist chamber, its point can be instantly returned to its previous exact position in the field.

The plate in turn supports and controls the motion of a second plate by attached guides which permit said plate to move in the line of the axial shaft of the MM, *i.e.*, at right angles to the axial line of the pipette, through a range of 2 inches. This movement is directly actuated by pressure applied to stud E and can be limited in its centrally directed course by an adjustable stop F.

This second plate is the base of the MM proper

[1] G. W. Fitz, "A New Micro-Manipulator," SCIENCE, January 15, 1931, 72–5.

and carries its fine adjustments G 1, 2, 3 as in the standard form. The cones of the fine adjustments for the transverse and vertical movements H, have, however, steeper pitches which increase their rapidity of action threefold. The speed of control in the axial line is also increased threefold by change of the actuating screw I. Long use has shown that these changes give the additional speed needed for pure culture work with no sacrifice of accuracy.

The adoption of felt linings for the adjustable guides of moving parts insures sufficient friction to maintain the parts in any desired position without special clamping, yet permits by its smooth resistance accurately controlled movement. The parts are thus always ready for the instant response required by this type of work. All jamming and uncertainty of control are eliminated. So smooth and accurate is the control that, for low powers, the coarse adjustments frequently suffice.

A special form of pipette holder J, which makes possible the quick change of pipettes, has been designed for this instrument. The pipette is held between two jaws. One is movable, is opened by lever K and is automatically closed by an adjustable spring. The other jaw is fixed but can be adjusted by means of a clamp to receive pipettes varying in diameter from 2 mm to 10 mm. The pipette holder is adjustably clamped to the end of the MM shaft; it can therefore be inclined from the horizontal to any desired angle.

The grouping of all controls, as in my standard MM, has been adhered to throughout. The direct correlation between the control movement and the apparent movement of the operating point in the field of the microscope, as well as the bilateral symmetry between the right- and left-hand instruments of the double assembly, are also that of the standard form. Reports from the instrument in use show that this convenience of construction and arrangement makes it possible to prepare and put away a mount of ten culture selections in 5 to 6 minutes. Of this time 2 minutes are required for the spreading of the cells.

A total weight of less than nine pounds for the double micro-manipulator, inclusive of its mahogany base, insures easy portability, even in a suitcase.

For valuable assistance in the clear definition of the requirements exacted by pure culture and microchemical work and in the testing out of the working models submitted by me I acknowledge my indebtedness to Dr. Morton C. Kahn, of the Cornell Medical Center, and to Mr. F. R. Swift, of the Fleischmann Laboratories, New York City.

G. W. FITZ

PECONIC, L. I., N. Y.

# SPECIAL ARTICLES

## ARTIFICIAL PRODUCTION OF RADIOACTIVE SUBSTANCES

CURIE and Joliot[1] have recently observed the emission of positive electrons from boron, magnesium and aluminum, for a considerable length of time after exposure to α particle bombardment. They suppose that in the case of boron, the boron nucleus captures the α particle and emits a neutron, leaving as the product nitrogen 13, which is radioactive, and subsequently emits a positive electron, becoming carbon 13. They suggest analogous processes for magnesium and aluminum, and point out that, if the process is of this nature, it should be possible to produce the unstable nitrogen 13 by bombarding carbon with deutons.

In order to investigate carbon and other elements under deuton bombardment, we constructed a disk, around which was fixed targets of a number of substances. The disk could be rotated inside the vacuum by means of a shaft so that it was possible to bring any one of the targets first into the ion beam for bombardment, and then into view of a Geiger counter having a thin window for recording the delayed emission of particles or gamma rays. Targets of LiF, Be, $H_3BO_3$, C, Mg and Al were subjected to bombardment, each for about 15 minutes with 5 microamperes deuton current, at 900,000 volts, and immediately rotated into view of the Geiger counter. In the case of boron and carbon, a large number of counts was recorded during the first few minutes after bombardment. Carbon gave the largest effect; several hundred counts per second (calculated for the total solid angle) and decreased at a rate corresponding to a half life of about 10 minutes. The effect from boron was somewhat less intense, and the half life was about 20 minutes. Other substances bombarded gave effects which were appreciable, but which might have been caused by carbon contamination on the surface of the targets. In the case of these small effects an investigation of the rates of decay will decide whether or not they are to be attributed to carbon.

To determine the nature of this delayed activity Dr. Carl D. Anderson and Seth H. Neddermyer placed a piece of freshly bombarded carbon in a Wilson cloud chamber and took a series of photographs, extending over a period of about two hours after bombardment. During the first hour each expansion revealed a number of electron tracks, nearly all of which were of positive polarity. In addition, a few short tracks ap-

[1] *Comptes Rendus*, 198, 254 (1934).

peared, which originated in the gas, indicating the presence of gamma rays.

The electrons ejected from the carbon appeared to have energies which were distributed from about 700,000 electron volts downward. It does not seem that the loss of energy of the electrons in getting out of the target can account for such a large proportion of low energy tracks. The carbon is essentially a thin target, since the depth to which it can be activated by the deutons is very small compared to the range of the ejected electrons. If it can be established, with certainty, that the energy spectrum of positive electrons emitted from nuclei is continuous, it will be of profound theoretical importance in dealing with the problem of the continuous negative beta ray emission.

Both the decay period (14 minutes) and the maximum energy of the electrons from carbon indicate that the active isotope here concerned is the same as in the case of boron bombarded with $\alpha$ particles, as reported by Curie and Joliot. A calculation on the basis of the length of time of bombardment and the half life indicates that one radioactive atom is produced for about $10^{10}$ deutons incident on the target.

We have previously shown that carbon bombarded with deutons emits gamma rays of hardness corresponding to about $3 \times 10^6$ volts, during the time of bombardment. This we associate with the emission of protons and the formation of $C^{13}$. Three gamma ray quanta are emitted in this process for about $10^8$ deutons at 900,000 volts. It seems, therefore, that when carbon is bombarded with deutons, not more than 1 per cent. of the transformations give as a product the radioactive $N^{13}$, while the remaining 99 per cent. give $C^{13}$.

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FEBRUARY 27, 1934

## THE PORTAL OF ENTRY AND TRANSMISSION OF THE VIRUS OF POLIOMYELITIS[1]

BOTH human and experimental poliomyelitis evince considerable evidence toward the belief that the virus of poliomyelitis enters the nasopharynx rather than the gastro-intestinal tract.

In acute cases the virus has been demonstrated in the nasal secretions or nasopharyngeal mucosa, where it may persist for some time. The nasal secretions of abortive cases, of healthy carriers and, on one occasion, the dust of the sick room, have been shown to contain the virus. In one instance, virus was found in the nasopharyngeal washings of a patient five days prior to the onset of symptoms. On the other hand, with the exception of one unconfirmed report, the virus has not been demonstrated in the feces of human cases. These facts and the epidemiology of the disease recently reviewed[2] suggest that the virus enters the upper respiratory tract and is spread by its droplets rather than by the excretions of the gastro-intestinal tract.

There is considerable experimental evidence to confirm these findings. In *Macacus rhesus* monkeys introduction of a virus-soaked tampon high up in the nares, injection of virus into the nasopharyngeal mucosa, application of the virus to the scarified mucosa or spraying the virus into the nose have all proved infective. Recently, several workers have reported consistent infection by the administration of multiple intranasal inoculations. We ourselves have a strain of "nasal" virus which gives uniform infectivity upon the administration of 0.2 cc of a 10 per cent. suspension into each of the upper nares. On the other hand, only three of the many who have fed the virus to monkeys have reported positive results. As yet the virus has not been recovered from the feces of poliomyelitis infected animals. Yet in such animals, it can easily be demonstrated in the nasal mucosa or nasal secretions, where it persists for a considerable length of time.

The propagation of the virus from its portal of entry to and through the central nervous system has not been studied extensively. There is evidence indicating that it may travel along the olfactory nerves to the brain. Virus has been demonstrated in the olfactory bulbs during the incubation period following the intranasal inoculation of virus in monkeys by Flexner and Clarke and Faber and Gebhardt. However, the latter authors were unable to follow the distribution of the virus throughout the rhinencephalon. Therefore, the transmission of the virus to the brain is in need of further study and, despite the evidence favoring the nasopharynx as the portal of entry, the small number of carriers and the low incidence of direct contact infections make additional proof desirable. From recent and careful review of the subject, the following is quoted:[2] "Although the pathological anatomy of poliomyelitis has received intensive study for a period extending over more than a hundred years, investigators have not reached an agreement with regard to the atrium of infection or the path of transmission of the virus within the organism. It is to be hoped that research will be continued along these lines until sufficient evidence has been obtained to permit of an authoritative statement."

In order to determine whether or not the virus

[1] From the Department of Bacteriology of New York University and Bellevue Medical School and the Department of Neuro-Surgery, McGill University, Montreal, Canada.

[2] "Poliomyelitis," International Committee. Williams and Wilkins, Baltimore, Md.

travels along the olfactory nerves, a bilateral section and partial removal of the bulb and tract was carried out on a series of *Macacus rhesus* monkeys through a transfrontal approach. In five experiments, these monkeys and controls with intact olfactory nerves were given nasal instillations of cord containing virus. In each case the control animals came down promptly, whereas the experimental animals remained well. In one experiment, whereas the three experimental animals withstood three inoculations given on successive days, two controls were paralyzed within short incubation periods after a single inoculation of virus. After this, the serums of the monkeys with cut olfactory bulbs were tested for antiviral substance, two of them against one infective dose of virus and the other against two infective doses. In no case did the serums neutralize; showing that the animals were in no way resistant to the virus. In a final experiment, two experimental animals resisted twelve intranasal instillations, whereas the control became paralyzed after a single injection.

The nasal mucosa is innervated not only by the olfactory nerves, but also by branches of the V and VII cranial nerves. In addition, virus can percolate from the nasopharynx to the tonsils with its intact nerve supply. Yet upon cutting the olfactory tract no infection occurred, indicating clearly that the first cranial nerve is the only one of the nasopharynx that can transmit the virus of poliomyelitis from the nasopharynx to the central nervous system. Over a period of three weeks, animals received twelve intranasal inoculations, of which considerable must have dribbled to the gastro-intestinal tract. This observation, together with the clinical and experimental data of others, discounts the gastro-intestinal tract as the portal of entry.

Whether the olfactory nerve affords the virus a passageway because it is non-medullated, or because its neurones lie in the nasal mucosa and are thus exposed to the virus, has yet to be determined. The fact that infection *via* the sciatic nerve succeeds only if it is injured, and inasmuch as injury to the nerve is followed by myelin degeneration, suggests that the lack of myelin may render the olfactory nerve vulnerable to the virus.

The next problem to consider is whether the virus spreads along the nerve fiber proper or its perineural lymph space. Inasmuch as the separation of the olfactory nerve fibers and their surrounding lymph spaces would involve considerable technical difficulty and since the lymph spaces drain into the subarachnoid space and the nerve fibers are continuous with those of the rhinencephalon, it was decided to determine whether the virus spread through the central nervous system by the spinal fluid or by the nerve tracts. Therefore, the spinal cords of monkeys were severed in the dorsal region and separated, the dura carefully closed and an intact flow of spinal fluid demonstrated over the gap. In one animal, virus inoculated into the brain failed to infect the lower segment of the cord, while virus inoculated into the lower segment did not penetrate above the point of separation. Infectivity or non-infectivity of the segments was determinal by presence or absence of microscopical lesion and demonstrable virus. These results, which are in keeping with the findings of Jungeblut and Spring, show that the spread of the virus through the central nervous system is along nerve tracts rather than by means of the cerebrospinal fluid and inasmuch as the perineural lymph spaces of the olfactory nerve continue as the subarachnoid spaces and the nerve fibers continue in the central nervous system, the virus must travel along the fibers of the nerve.

Besides demonstrating that the virus travels along nerve fibers as Hurst and Fairbrother, Jungeblut and Spring, Faber and Gebhardt have suggested, the above experiment indicates that there is not hematogenous spread of the virus, for the blood supply was intact for each segment. It appears, therefore, that experimental poliomyelitis is a disease of the central nervous system exclusively, as the pathology and the difficulty of demonstrating virus outside of the central nervous system indicate; whether the human disease is as entirely neurotropic as the experimental disease or whether the latter has acquired this property through continued passage is being investigated.

It has been shown that experimental poliomyelitis is entirely neurotropic and that the virus travels along the olfactory nerve fibers to the central nervous system, where it is propagated along the nerve tracts. Since only the olfactory nerve of the nasal cavity can carry the virus and because no infection was obtained when large amounts of virus reached the gastro-intestinal tract, the portal of entry then must be the nasal cavities.

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## BOOKS RECEIVED

*Carnegie Institution of Washington Year Book No. 32, July, 1932–June, 1933.* Pp. xix + 388. The Institution.

CHILDS, W. H. J. *Physical Constants.* Pp. viii + 77. Dutton. $1.20.

OGDEN, C. K. *The System of Basic English.* Pp. ix + 320. Harcourt, Brace. $2.50.

POUND, ARTHUR. *The Turning Wheel.* Pp. xvi + 517. Illustrated. Doubleday, Doran. $3.50.

SOKOLNIKOFF, IVAN and ELIZABETH S. SOKOLNIKOFF. *Higher Mathematics for Engineers and Physicists.* Pp. xiii + 482. Illustrated. McGraw-Hill. $4.00.